RESEARCH MANAGEMENT

A PUBLICATION OF THE
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ABOUT THIS ISSUE

"The refinement of techniques is the last gasp of a dying civilization." This observation of Splengler is quoted in our first article which deals with the necessity for new support of basic investigations, and for the greater encouragement of audacious innovations on the part of those who have the capacity for such contributions. The author, Arthur Bronwell, is President of the Worcester Polytechnic Institute. He decries the growing reliance of universities on Government support of research programs. He calls upon Industry to reverse the tendency, for our traditional system of free enterprise is at stake. In support of his thesis, Dr. Bronwell offers powerful evidence including an eminent example from his own institution. His paper is entitled "Strengthening the Citadels of Research."

The Spring 1960 meeting of the Industrial Research Institute had a panel session devoted to the topic "Budget and Cost Control for Research and Development." The preceding issue of this Journal reported the presentation of R. W. Olson of Texas Instruments. We now record the substance of the talk given by T. L. Wilson of United States Rubber Company. The System in effect at the Research Center of that organization provides answers to questions such as: How much did a research project cost? What was the cost of each element of that project? What is a fair estimate of the cost of a project which is contemplated? All of this information and more become available quickly and at reasonable expense. Dr. Wilson is manager of the Research Center. His paper appears as our second article.

One thing a research director should not do is to direct research. Rather, his job is to lay the plans and provide the leader-

ship to assure optimum productivity over the whole R and D effort ranging from fundamental research to commercialization. Between those extremes lies a wide gap which, however, can be bridged by skillful efforts consciously oriented toward company goals. Our third article, "Planning for Research: The Problems Involved," gives not only a keen analysis of the problems, but one may also discern suggestions as to the best means for resolution. The author is Robert W. Cairns, Director of Research of the Hercules Powder Company. His article constitutes the substance of the presentation he made on September 13, 1960 at a symposium of the Division of Chemical Marketing and Economics of the American Chemical Society.

The development of the managerial potential of creative specialists is essentially an on-the-job function. Three steps are involved: orientation, guidance, and periodic appraisal. The first step is preliminary, its duration short. The second provides a more extended means of acquainting the new supervisor with the outlook and techniques he needs for discharge of his responsibilities. The third, periodic appraisal, continues throughout the manager's career. In all three phases, the role of the supervisor's superior is paramount, for his talents in the training of managers reflect markedly upon the results achieved. Our fourth article presents an interesting study of a thesis that managers can be made —they need not be born. "Developing Managers Out of Creative Specialists" was written by Stephen R. Michael, Systems Unit Head at Smith Kline & French Laboratories.

In this issue, we publish the fourth Proceedings of the Research Management Study Group Conferences, this one entitled, "Optimizing the Relationship Between Research and Marketing." The two study groups on that subject were led by Lyle I. Gilbertson, Administrative Director, Central Research Laboratories, Air Reduction Company, and Erwin G. Somogyi, Vice President, General Manager, Research and Engineering Division, Monsanto Chemical Company. The conferences were characterized by lively, informal discussions in which all participated and all enjoyed. A

summary of the observations which were made and conclusions reached is presented here as a staff report.

The scientific method does not stop where logic ends. It must venture further, penetrating the areas of intuition and imagination where indeed the discoveries of greatest consequences have originated. The creative man who strikes out into new fields does, however, place himself in a precarious position, for while his work may bear rich fruit, it is also quite as likely to yield a barren harvest. He needs encouragement to renew his efforts when things go wrong. Our sixth article, "Stimulating Creativity in Research and Development," discusses novel and important aspects of the problem and recommends measures which operate to the advantage of the organization as well as the scientist. The author is R. B. Mears, Assistant Vice President for Applied Research, United States Steel Corporation.

As mentioned in the preceding issue, the Board of Editors has adopted the practice of publishing reviews of books. However, the two which we now present go beyond the ordinary book review; they merit the descriptive phrase "Commentaries on New Books." C. P. Snow's "The Two Cultures and the Scientific Revolution" is discussed by N. A. Finkelstein, Vice President and Director of Research, General Dynamics/Electronics. The other Book, Simon Marcson's "The Scientist in American Industry" is analyzed by I. C. Ross of Bell Telephone Laboratories. The Board of Editors would appreciate readers' comments and suggestions on this feature of our journal.

STRENGTHENING THE CITADELS OF RESEARCH

ARTHUR BRONWELL*

President, Worcester Polytechnic Institute, Worcester, Massachusetts

Not long ago one of the exceptionally able members of our faculty prepared a research proposal for consideration by a large corporation which has active research interests. It involved a study which the individual regarded as being highly significant and which might open up opportunities for appreciable commercial endeavor. The proposal was a modest one, as research proposals go, and involved a bare minimum of financial support. In fact, I was somewhat concerned because I did not believe that our costs were covered. After careful consideration, the company rejected it.

About this same time, another member of our faculty prepared a proposal for research study of a project which he too thought was highly important. This proposal, submitted to a government agency embodied a much more liberal budget of some $25,000 which made provision for the part-time salary of three students as graduate research assistants for a period of two years

* Dr. Bronwell has a distinguished record of achievement in education and research. A graduate of the Illinois Institute of Technology, he became an instructor at Northwestern University in 1937 and subsequently a professor of electrical engineering. During World War II he organized and directed a signal officers' training school for the Army and a research project on radar. He is the author of numerous technical papers, and a book "Advanced Mathematics in Physics and Engineering." He served as Secretary of the American Society for Engineering Education and was editor of its journal. He is now President of Worcester Polytechnic Institute.

and also a modest amount of research equipment regarded as essential. The $25,000 project was accepted and is now effectively under way. Out of this will probably come several master's degree theses, published reports on research accomplishments, and the advancement of education of two fine young men, as well as advancement of the professional competence and stature of the faculty member directing the project.

This is a homely little incident. But I believe that it typifies a large national problem. It has become, I believe, a fairly universally accepted doctrine among colleges and universities that when research money is needed it should not be sought from industry. If a company has a specific problem which it wants solved and which happens to fit in with the commercial objectives of the company, then it will come to a college for help. But the reverse situation—that of colleges and universities obtaining research funds from industry for the conduct of basic research has almost universally met with failure.

Why is this? Certainly industry has everything to gain by a large scale attack with the colleges and universities spearheading the pursuit of fundamental knowledge. Industry itself, generally speaking, is not geared for this kind of fundamental research. A few companies have successfully undertaken fundamental research programs, but industrial research is usually concerned with the obtaining of specific knowledge for specific goals relative to commercial products or new manufacturing processes.

There is a sharp lesson to be learned from our experience of the last two decades. Time and time again it has been clearly demonstrated that research of a fundamental character can leap right over present-day technologies and develop entirely new technologies which go far beyond the day-to-day developments. Industrial research is essential, of course, since this is the very process whereby our scientific and technological knowledge is fitted into useful products for society's benefit. It requires massive effort in

innumerable directions to gear our manufacturing processes to constantly changing goals.

But there is also the other aspect—that of advancing our scientific knowledge in fundamental directions, which can open up whole new frontiers of industrial endeavor. Many of the most rapidly growing companies today can boast not only that their technologies were unknown 40 years ago, but that the very scientific principles upon which they are founded were totally unknown at that time. In the jet propulsion of airplanes, in studies of the movement of electrons and holes in metals, leading to transistors that threaten to supplant the vacuum tubes in all of our communication systems, in the avalanche that has descended upon the textile field in the form of synthetic textiles, in the astounding developments in plastics with their enormous versatility, in the development of new materials of all kinds with unique properties we have seen that revolutionary new developments can emerge from fundamental studies in science. Not all of these came out of the colleges and universities. Certainly the industrial laboratories have contributed enormously to this advancing front of new knowledge. But the colleges and universities do hold enormous potential, and they can make contributions in a way which is quite different from that of industry.

Currently when we in the colleges need research money we rush to the government. Frankly, there is no other source. A college or university, to be worth its salt, has a number of faculty members who individually have a genuine, deep-seated, and dedicated desire to be a part of the advancing front of knowledge and to contribute to its advance. This ambition must be encouraged, for this is the very essence of a dynamic society. If our colleges and universities become static and oblivious to the growing opportunities of the future, then there will be little to sustain a dynamic spirit in society itself.

Why is there this seemingly insurmountable gulf separating industry and the colleges in the matter of support of fundamental research? There are many reasons. First, I suspect that the com-

panies have had about all that they can do to establish their own research operations, and they have not yet reached the stage where they can support fundamental research in colleges and universities. Then too, government money is available for research in colleges and universities which, in part, supplies the need. Perhaps a company might feel that there is no fun in being a small fish in a pond dominated by a whale. Why should a company put its money where government has already pre-empted the scene?

Many executives in industry do not understand fundamental research. They regard the slow germination of ideas in a college atmosphere as irresponsible daudling. Some of it is. But the uncertain, faltering search into unknown fields, which requires a greater depth of exploration of new knowledge, is seldom as efficient as industrial research, which is directed toward well-defined objectives with clear-cut approaches and a commercial product at the output end. But experience has clearly shown that we must not sell this kind of research short.

I believe that we will come to the time in the not too distant future when industry and the colleges must and will cooperate much more closely and effectively in the pursuit of fundamental knowledge, and that industry will provide a very substantially increased portion of the funds necessary for this kind of research. Perhaps this may require special tax incentives. But there is a natural bond of affinity between industry and the colleges and universities in our nation which is developing on many fronts and, unquestionably, will develop more fully in the areas of fundamental research.

In 1951, I served on a United States Mission to Japan. The Mission was at the request of our occupation government, which at that time was seriously and quite legitimately concerned about Japan's achieving an industrial recovery at a sufficient rate to stave off Communist encroachment. Since engineering education was regarded as a pivotal point in industrial recovery, the mission was requested. In Japan we found that virtually an impenetrable wall existed between industries and the colleges and universities. Our

conferences with Japanese industrialists and educators were the first experiences in which engineering educators and people from industry met in common forums to discuss their mutual problems. This condition arose out of the fact that most universities are federally financed and federally controlled. Civil Service laws do not permit faculty members to undertake outside consulting for private remuneration. Consequently, faculty members had no interest in going into industry and people in industry often had little other than contempt for the faculty.

It became clear to me from this experience that we in the United States have progressed a long way in bridging the gap between industry and the colleges and universities. Not only do our colleges and universities educate the people who provide the prime moving force for industry, but they are also involved in a wide variety of evening and graduate study educational programs which have been quite responsive to the needs of industry. Financial assistance in the support of research to advance our fundamental knowledge, not only in the sciences and the technologies, but in all other fields of intellectual endeavor as well, can contribute enormously to our social and industrial advance.

A recent study by the National Science Foundation has shown that only 4% of the basic research funds in the colleges and universities emanate from industry. Government accounts for approximately 63% of the research funds. Of the twelve billion dollar research budget of the nation, the colleges and universities account for roughly one billion dollars. Industry expends 70% of the total, but pays 50% of the cost, the balance of industrial research being supported by the government.

If industry's contribution to the colleges and universities for fundamental research were to rise to, let us say, 8% of its own research expenditures, this would profoundly accelerate our progress in advancing science, technology, and fundamental knowledge in all fields. I am not speaking of support for specific product development. It would be a serious mistake for industry to saddle the colleges and universities with too much of this kind of

research, for it would divert capable talent from the pursuit of knowledge which could in the aggregate have far greater impact upon our technological advance than product development. Rather, I am speaking of research of a character which would be largely defined by the faculty members themselves along the lines of their own interests and in directions which they feel might yield highly significant new knowledge. This must be supported either by unrestricted funds or by funds which have only the broadest limitations placed upon their use.

Over half a century ago a student of Worcester Polytechnic Institute was daydreaming. He wrote that some day rockets would travel to the moon and that they would be powered by atomic energy. So dedicated was he to this ideal that he devoted his entire lifetime in the pursuit of this one objective, despite the fact that his intimate associates regarded this as nothing other than playing with glorified fireworks. He worked with solid and liquid fuels, developed a high efficiency rocket gun, developed pendulum and gyroscopic stabilization of rockets, worked with multi-stage rockets, and developed the aerodynamic properties of rockets in flight. Dr. Robert Goddard, according to Dr. Wernher von Braun, made virtually every major discovery in rocketry, yet his dogged persistence and enormous achievements were unheralded in his lifetime. Today we realize that his discoveries not only make it possible for man to project himself into outer space and to travel to the planets, but what may be of far greater consequence, it has opened up whole new domains of fundamental study in astrophysics, astronautics, astrochemistry, the exploration of the geology and life on other planets, as well as the cosmology of the universe so vast in extent that we cannot possibly comprehend its consequences today. Irresponsible daudling? Perhaps in Goddard's day. But certainly today a scientific study of the most profound importance, which makes it possible for man to leap way ahead in his quest of knowledge and the advancement of his technologies.

I need not recite the potentialities of such epoch-making

breakthroughs as the breaking apart of the nucleus of the atom, leading to atomic energy and vast new domains of nuclear science with untold consequences; the experimentation of the curiosities of long chain polymers which led directly into the discoveries of nylon, dacron, and other materials; the development of theories of servomechanisms which opened up a whole new science underlying automation; and countless other explorations into fundamental knowledge which were initiated in college and university laboratories and which have had revolutionary consequences.

Perhaps the reticence on the part of industry to support college research is based on the commonly accepted belief that government now provides adequate funds for fundamental research, so why should industry enter onto the scene? First, I should like to state emphatically that federal funds are not now and never have been adequate to handle the job that is needed in accelerating fundamental investigations. But even if the funds were adequate, this belief misses the point completely. We are faced with the fundamental question in this country of whether our colleges and universities should resign themselves to the totality of government support as the source of the advancement of all knowledge. Personally, I find this idea repulsive, yet I cannot escape the conclusion that this is not only a dominant trend, it is a trend of avalanche proportions. The question goes far beyond the purely utilitarian viewpoint. It involves the very basic question of whether or not we in the colleges and universities and you people in industry can be ingenious enough to devise ways of developing parallel channels which will preserve our free enterprise approach in the pursuit of knowledge. When we close up shop and send our stock and trade all down to Washington, we are all done with democracy.

Recently, we held a conference at Worcester Polytechnic Institute, supported by the National Science Foundation, which focused on one particular problem—the question of how we can lift the sights and capabilities of more of our talented young people so that they will seek larger goals in their research endeavors.

Researchers have a strong propensity to gravitate into well-populated fields where there is a convenient assortment of technical meetings and publications, as well as an alluring community of interest which beckons the young researcher into the fold. Much of our college and university research is devoted to piecing out small voids in knowledge, in mosaics which are already quite well defined. Some individuals have that rare faculty of leaping over these well-travelled fields into new areas which hold promise of great scientific or technological discovery. But these individuals are few and far between. Stop a moment and count the number of individuals in your own company whom you would regard as being real pioneers, in that they have the faculty of bringing forth innovations of major consequence. Those who participated in the conference agreed that here is a problem of major consequence not only to colleges and universities, but to industry as well; a problem which vitally affects the pace of advance of our whole emerging front of scientific and technological progress.

The conference participants included leading research executives in industry, college presidents, and deans of engineering. They pointed out that our college educational programs are not creatively oriented. Yet, the creative experience is the very essence of successful creativity. Education succeeds quite well in dodging the creative issue by a total preoccupation with the acquisition of knowledge and the mastery of analytical techniques, these being regarded as of prime importance. The creative experience is likely to be an inefficient process because invariably it involves bungling and time-consuming efforts. Most students never get very far by this process. Yet a much more creative outlook is essential if we are to raise the level of creative competence of our young people.

But the conference made other recommendations which I believe throws new light on the problem. It urged that the scientific and engineering societies undertake a new mode of operation —that they provide a forum in which leading scientists and engineers would be invited to project ahead their ideas as to what is coming up over the horizon. This forum for the philosophy of

ideas, one might well imagine, would attract eager and energetic young researchers and, unquestionably, it would trigger some of them off into new adventures of great consequence.

The greatest poverty, the conference concluded, is not the poverty of ambition, inferior capabilities, or sluggish conformism. Rather, the greatest poverty is the poverty of ideas. We have no effective link, no mechanism for bringing our talented young people into stimulative, imaginative association with the leading thinkers of our times in projective, imaginative interchange of ideas. The professional societies operate largely in the present and the past; they have little to do with the future. A researcher can report on completed research, but seldom do we find even outstanding scientists projecting ahead philosophically and speculatively.

It is a sad commentary that Buck Rogers and science fiction were ten years ahead of our engineering colleges and our professional societies in taking hold of the space problem. It will always be like this, unless we can find an effective forum for the propagation of new ideas outside of the well-travelled concourses.

Time and again new professional societies have been created by individuals who have smarted under the binding orthodoxy of existing professional societies, which look upon new budding fields as unwanted orphans. In fields such as electronics, rocketry, space travel, nuclear science, and computers, we have found a virtual revolution as one after another society has broken loose because this was the only way that they could get that degree of freedom to soar in their own ideas and efforts. It is not enough, the conference concluded, for the professional societies to merely "adjust" after the pioneering work has been done. They should develop positive programs which give full force to pioneering endeavors in the very earliest stages, and this often requires philosophical, speculative thinking about the future, for at this stage, nothing is certain. A professional society should have that priceless quality of open and challenging minds exploring the distant goals. It is in this kind of

atmosphere that youthful talents will rise to great heights of ambition and achievement.

It is a foregone conclusion that new centers of scientific and technological endeavor coming up over the horizon probably will not fit into the existing structures of our engineering societies. Many of these are in the limitless expanses of the interdisciplinary fields—biophysics, biochemistry, medical electronics, nuclear science, rocket propulsion, computers—the vast no-man's land which finds itself an outcast of the principle professional societies. The slow diffusion of science into engineering unquestionably has been caused, in part, by the difficulties in getting effective cross fertilization of ideas between different disciplines, and by the fact that inadequate forums have existed for this dissemination of new ideas.

Again and again history has shown that great teachers can profoundly influence the opening up of a whole new field of intellectual or creative endeavor. Invariably, these are people who have fertile, imaginative minds and who are inspired to great heights of personal effort and achievement. But we have not yet found effective means of bringing talented young people into stimulative association with these leaders in scientific and technological thought in a projective, philosophical sense, except as this may occur in the very limited personal associations as student or understudy.

This conference had a highly salutary influence on the thinking in the professional societies. Its recommendations were adopted by vote of the Council of the American Institute of Chemical Engineers. Another step was taken by the Engineers' Joint Council in appointing a committee to project ahead in trying to identify areas where major research effort might lead to large-scale consequences. Other engineering societies have undertaken new kinds of programs in which leading scientists and engineers are invited to assess the future as they see it.

The essence of creativity is an environment of freedom, encouragement, and opportunity. Freedom of the individual to

try out his own ideas, encouragement to reach for the larger and sometimes more distant goals, and opportunity to gain that matchless inspiration of association with leaders in thought and accomplishment. Structured research programs so often erect immutable barriers to this kind of originality of thought. Freedom? Yes, but bounded by the objectives of the particular project.

Research is relatively new on our national scene. The vigorous promotion of research by the colleges, industry, and government during the past two decades has been one of the most revolutionary innovations in American history. But with the growth in size and complexity of research, there have arisen problems of great consequence to our nation's scientific and technological progress which are deserving of critical and objective study.

"The refinement of techniques," said Spengler, "is the last gasp of a dying civilization." Our success in averting this "last gasp" depends upon whether or not we in the colleges and you people in industry can find effective ways of lifting an adequate number of our talented young people above the level of the "refinement of techniques" to the stage where they can develop the kind of originality of thought and brilliance of achievement which will blaze new trails in mankind's never-ending search.

BUDGET AND COST CONTROL IN RESEARCH AND DEVELOPMENT

T. L. WILSON*

Manager, Research Center, U. S. Rubber Company, Wayne, New Jersey

This paper describes the budget cost and control system in use at the Research Center of the United States Rubber Company. The Center is in Wayne, N. J., having moved there in 1957 from Passaic where we were located for 28 years. It is in a suburban area about 20 miles from the company headquarters in New York City. It comprises five major buildings with a total floor area of 175,000 square feet. The total land area is about 100 acres.

The Research Center houses the Research and Development Department, a staff organization the head of which reports directly to the top management. The Research and Development Department performs approximately 25% of the company's research and development activities; the remainder is conducted by organizations within the various operating divisions. The Center does basic and applied research in chemistry, physics, radiation, and engineering with particular reference to the fields of natural and synthetic rubber, chemicals, plastics, and fibers. Our cost control system in its present form has been in effect about three years.

* Thomas L. Wilson has a doctorate in physical chemistry from the University of Chicago. His professional career has been with the United States Rubber Company. Starting as a research scientist, he subsequently filled the positions of administrative assistant, department head, and Manager of the research center which is his current assignment. He is active in professional societies and in community affairs at Wayne and Montclair, N. J., where he resides.

Objectives and Principles

One important objective of any cost control system is to insure that you do not run out of money. This, in our opinion, is not the major consideration, for that condition can be overcome simply, although frequently at the expense of overall efficiency, by stopping operations when the money is expended. Basically, a budget reporting system should tell: where you are in the research and development program; whether you are moving in the directions that make sense, and whether your activities are being directed in a way which will give the best results at the lowest cost.

Research and development work reminds one of driving a car. Unless you have good brakes you will get into trouble. But the steering wheel, the accelerator and the gas gauge are equally important controls. Without them you cannot have much success in getting where you want to go.

Any good budget control system should provide management with quick, accurate answers to significant questions and give these answers without undue cost in terms of money or manpower. The basic principles of our present system were worked out for us by the corporate Treasurer's department to provide the kind of answers needed. The questions of most concern are:

(*1*) How much did a specific research project really cost? This is an important question. Today no business can grow or survive without a pretty realistic picture of the true costs of its principal products. Research projects are after all performed to obtain results which are the principal products of a research laboratory.

(*2*) How much did a particular operation or service cost? We need an answer to this question for two reasons: (*a*) to help us decide whether we are spending too much or too little on these activities, and (*b*) to guide us in decisions to expand or reduce the activities in line with proposed research programs.

(*3*) What will a particular new activity or project cost us in the future? This is the really vital question. The first two ques-

tions are concerned with history. Interesting as it may be, history itself is of little value unless it provides a sound basis for present action and future results. We are primarily interested in the historical costs of projects or functions because we need this information to help us foresee how much money and manpower must be allotted and how they should best be deployed.

A satisfactory budget and cost control system should operate in such a way as to supply answers to these and related questions and require as little as possible of the research worker's time. Further, we did feel that there could be some real advantages to be derived from making available to the responsible heads of each of our operating research units, control information on their own projects and operations.

Applying the Principles

Figure 1 shows, in the form of an organization chart, the basic structure of our Research and Development operations. Each one of the blocks represents an operating entity or group within the whole organization. The general functions of the departments are classified as Direct Research, Staff Services, Technical Services and Facility Services.

A Research Department Manager is responsible for directing the activities of certain technical and supporting manpower in the conduct of his research projects. Each operating unit utilizes equipment and facilities which are provided as part of the overall operation, and occupies a certain space.

A prorata share of what are usually referred to as "fixed costs" are assigned to each one of the operating units on the basis of the area it occupies. We assign to each department its prorata share of the cost of certain essential services, such as steam, gas, and electricity. The distribution of these costs is based on general engineering estimates. Other indirect costs are assigned to each department on the basis of the total man-hours available.

In our operations we have three technical service departments

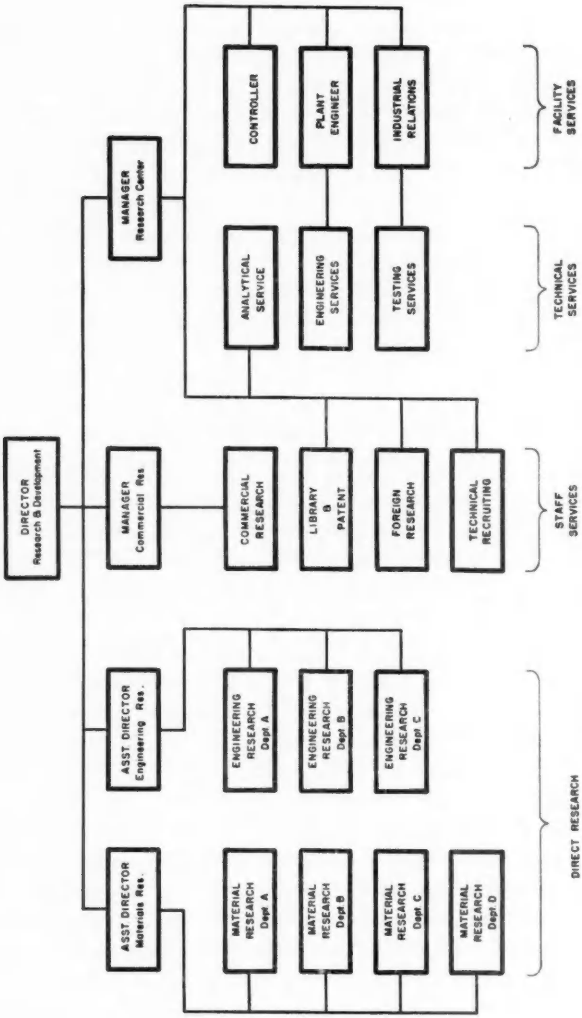

Fig. 1. Organization of Research and Development Department.

which perform chemical and physical analyses, physical testing and engineering services for the direct research departments. All such work is done on job orders and billed to the applicable direct research departments at actual cost.

The Mechanics of Producing Budget and Control Records

Each of the direct research departments has a certain number of specific projects on which it works. Costs are distributed to these projects on the basis of the direct hours charged to the specific project. At the end of every month all of the operating units, whether direct or indirect, are given copies of their cost summaries prepared in the control department. One sheet shows the total amount and composition of their internal operating costs. The other sheet summarizes charges to projects.

For direct research departments these include cost transfers from either technical service departments or with other direct research departments. The project cost sheets report differences between project budgets and actual cost on a cumulative basis. Figure 2 shows an internal departmental cost sheet and a project cost sheet for one department. Summary totals of these expenditures together with the departmental cost sheets are submitted to the Research Center management. All of this information is compiled in our control department. The only obligation that the direct research worker has is the preparation of his monthly time distribution sheet; the rest is done by the control department.

In order to make it possible to get breakdowns of costs by research projects, the control department enters the information on standard McBee punch cards so that actual costs for a specific project can be accumulated at any time in very short order. With our present system, we can get the cost of specific projects in about 15 to 20 minutes. Figure 3 shows the forms used in assembling these cost records.

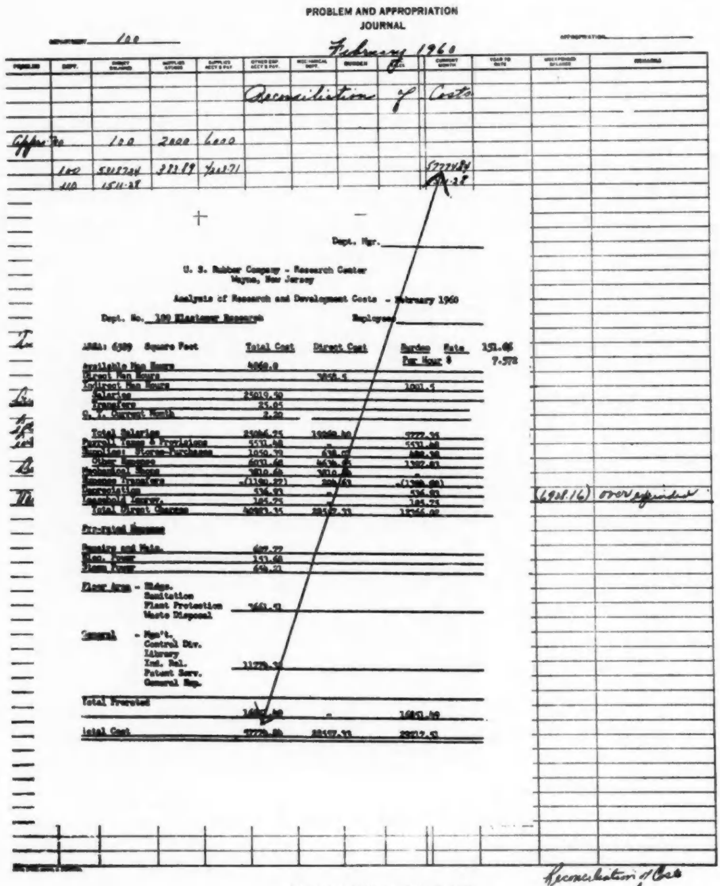

Fig. 2. Research Department Cost Sheet.

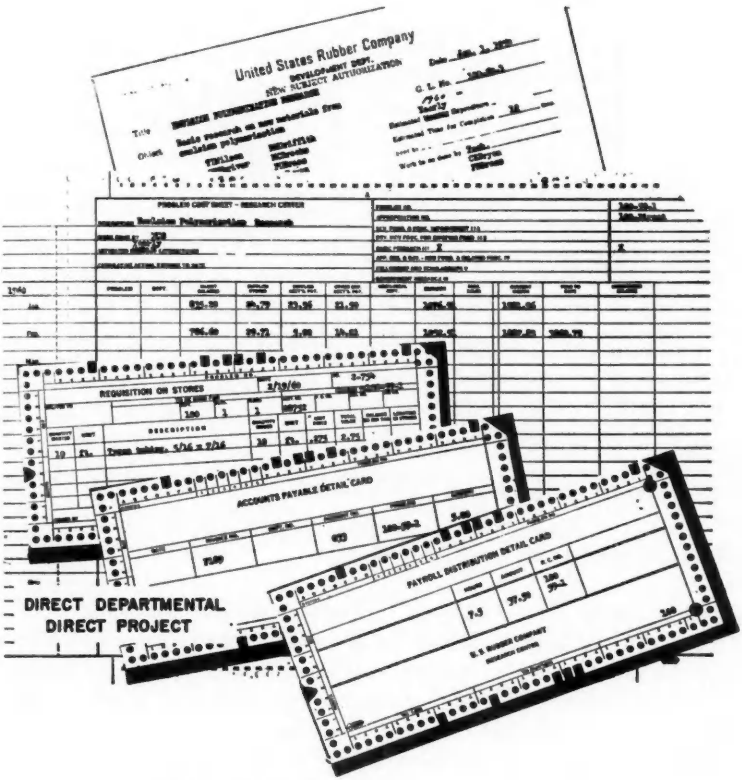

Fig. 3. Forms Required for Cost Control.

Using Cost Control Records in Planning

Figure 4 shows the trend of accumulative costs for the whole research operation for the year 1959. Direct charges account for 49.5% of total cost and indirect for 50.5 of total cost. Direct charges include not only direct salaries but all direct costs which can be properly allocated to specific research projects. The distri-

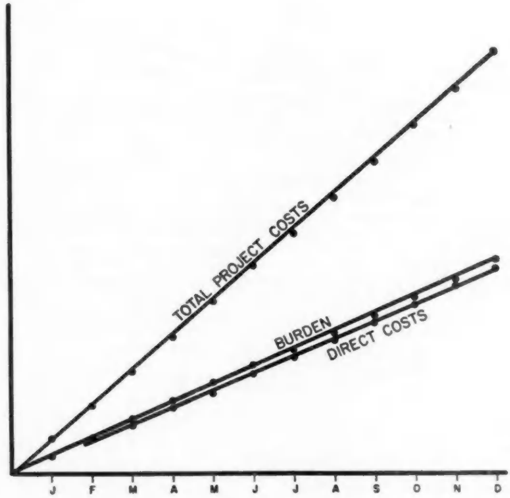

Figure 4. Cumulative expenditures on all projects.

bution of costs in research departments will vary, depending on the nature of the work. We have found that an analysis of this distribution is very helpful in forecasting cost on specific projects.

The accumulative plot of total costs, direct and indirect costs for a typical Materials Research Department, follows almost the same pattern as the laboratory operations as a whole. If we were to use the overall laboratory average cost per technical man as a basis for forecasting the probable cost of increased activity in this kind of work, we would not be far off. This is, however, not true of all departments, particularly those whose work is of an engineering nature.

Figure 5 is the accumulative record for a research department which is primarily concerned with engineering projects. Using the overall average as a basis for projecting probable costs in this area would lead us far astray. The reason for this departure can

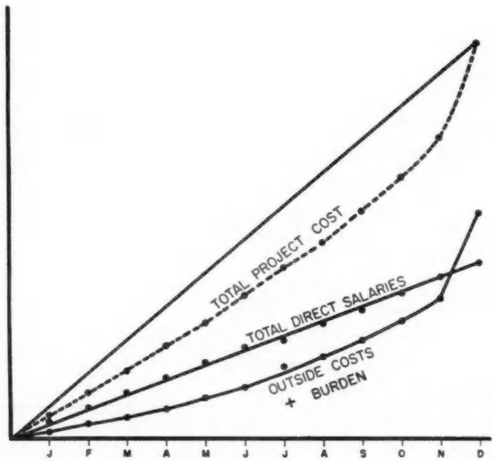

Figure 5. Typical annual project cost for an engineering research department.

be clearly understood if we consider how engineering projects move.

There is first a planning and designing stage where most of the costs are internal direct labor costs. Usually after a planning and designing period, decisions to build equipment or purchase hardware are reached. The work is generally done by outside contractors, and the costs do not begin to appear until the equipment or the hardware is delivered. We have found that this is a common pattern for engineering work and must be recognized in planning and expanding effort in such a field.

Another type of operation is illustrated by Figure 6 which is the cost record for one of the service departments. Here the costs for equipment and facilities are relatively high as compared with the cost per man-hour of direct work. Nevertheless, these costs must ultimately be distributed in some way to the project cost. In planning future programs, the possible expansion of technical service facilities must be considered.

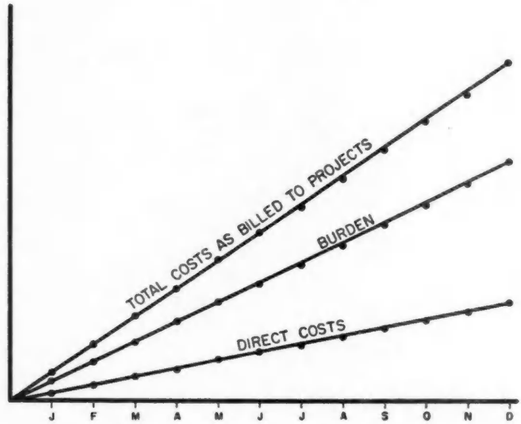

Figure 6. Typical annual cost for a technical service department.

Cost of Operating the System

The Controller's Department in our organization has a wider area of operations than the title might indicate. In addition to the normal control department functions, it administers purchasing, receiving and shipping, stores and stockrooms, and provides glassware washing and general office service to the whole Research Center. Each of these functions is treated as a cost center and carries its proper share of fixed and direct costs. An analysis of a typical year's costs for the Controller's Department would show the following:

	% of total research center costs
Control & Accounting	1.5
Purchasing, receiving, & shipping	1.4
Stores including all general supplies	0.9
Glassware washing & office service	1.1
	4.9

To summarize:

(*1*) We consider each operating unit as a definite cost center.

(*2*) We relate all of the indirect costs to the direct projects on a realistic basis.

(*3*) Our background experience provides basic factors useful in forecasting costs of increased effort in specific areas.

(*4*) Our overall research budget is presented in terms of project costs, and the control information is accumulated in such a way that, at any time, we can compare actual costs with our forecast.

(*5*) We can accumulate these costs against a relatively small number of projects to form the basis for review to decide whether or not increased or decreased emphasis is in order.

(*6*) We can provide this type of budget control information without any increase in our control section staff and without any additional burden on the direct research people.

(*7*) The background cost information which we give to the managers of the specific direct research departments helps them to do a better job of directing their activities.

(*8*) The total cost for operating our control and accounting section is less than 5% of our total cost of operations, and we obtain information that is of considerable help in planning our overall research programs.

PLANNING FOR RESEARCH: THE PROBLEMS INVOLVED*

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The subject of this discussion is not the problems on which research is done, but rather the problems involved in planning (or in some cases, in *not* planning) for industrial research and development. In thinking about such problems, we are immediately confronted with two major dichotomies. First, there is the dichotomy of individual creativity versus team effort; volumes have been written on each, and each is essential for successful industrial research. How then can we simultaneously take account, in research planning, of these seemingly disparate elements?

The other dichotomy in the industrial research picture is imposed by the apparent incompatibility of *scientific research*—a free-

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roving and analytical inquiry into the domain of unpredictable phenomena and *commercial development*—the ultimate business outcome of successful industrial research, with its practical aura of economic and company values and restraints.

Perhaps we are dealing here with only *one* essential dichotomy, the one which arises from the transitional nature of industrial research. Successful commercial research can be represented by an orderly progression from basic to applied research, thence through development to commercialization where the payoff is realized. At one extreme, there is the cloistered pursuit of natural phenomena and the laws governing them by scientists who are frequently as sophisticated in research techniques as they are detached from the practical facts of business success. On the opposite extreme is the commercialization phase, in which the business opportunists eagerly pounce on any newly discovered technical advance which shows promise of surpassing the competition. We can exemplify these conflicting aspects of research and development either in personal or impersonal terms; looked at in either way, the resolution of this conflict is the all-pervading problem of research planning. Let us consider the key problems involved at each phase of this research progression.

Basic Research

I am glad that the title of this symposium mentions planning *for* research, thus avoiding the controversial implication that research itself can be planned. At the scientific end of the spectrum, we must support the spirit of free inquiry. Specifically, the question is how to deal with the unpredictability of the results of scientific investigation which must underlie all applied research and practical development. At this point, we can conceive of no better way to plan for the unpredictable course of the work than through wise selection of the individual research scientist and through careful attention to his training, continued support, and orientation to the long-range interests of the company.

A great deal has been said about the selection of creative scientists who can best perform the job of research which we have characterized as basic or exploratory. I think we will all agree that there are types of scientists who are particularly interested in and particularly well equipped for prospecting research—those who are essentially self-starters and clearly demonstrate their capacity for seeking new ideas and initiating new approaches. Likewise, there are those who seem to have a special alertness toward pointing unexpected results in a useful direction. It is almost axiomatic that a good basic researcher is not rigidly bound by his initial concepts. There are outstanding examples of reactions or scientific studies that did not work out, but instead, by chance, revealed a new and unexpected type of behavior. Such "chance discoveries" are most likely to come to those who are actually experimenting in a field of interest to them and about which they are well informed. And it is usually *not* by chance that the investigators perceive these deviations and recognize their inherent significance.

For example, in early studies on reactions of gases under extreme pressure, investigators of the Imperial Chemical Industries found that vessels were being contaminated with a solid deposit. The unexpected deposit was traced to polymerization of ethylene. Subsequently, the reaction was developed to provide a new material having outstanding insulation properties, just in time to fill an essential need in Britain's World War II radar defenses. Such a dramatic instance may seldom occur in most basic research studies, but significant advances do result from the intelligent handling of unexpected results. In our laboratories at the Hercules Research Center, one of our analytical research chemists had the job of investigating suitable materials for the handling of bis(chloromethyl) oxetane. His keen observation of the unexpected effect of metallic contaminants led to a considerable improvement in the polymerization process from which our Penton resin is derived. The initial observation and its follow-up required something more than the skills of a good analytical chemist.

Traits of character that are generally recognized as important

in the selection of the basic scientist, to insure optimum use of his scientific skills, have been variously described. We say that he has an insatiable curiosity for the facts of nature, or that he has a thorn in his side which incessantly forces him to seek out new information in unknown areas; thus we have the problem of seeking out the people who are readily spurred or inspired into scientific creativity.

Another problem is to convey a sense of individual responsibility to such industrial scientists; through better communication and understanding, to let such people establish a dignified basis for their exploratory research. However impractical the scientist may seem to the more business-minded managers, and however detached the scientist may be from the ultimate practicalities involved, understanding by his supervisors, prestige among his fellows, and rewards commensurate with his particular contributions are factors which can help to encourage continued good performance and personal growth as the industrial scientist matures.

One may cite a hypothetical example of a basic research laboratory supported by a company with a management enlightened as to the values of scientific accomplishments and the long-range importance of basic research. Such a laboratory might be led by scientists with great technical skills and well-earned scientific reputation. Adopting a seemingly beneficial hands-off policy, the company executives might leave the determination of fields for research, as well as project selection entirely up to the scientific leaders. Such an apparently happy state of affairs would seem to be in complete accord with some views we have heard as to the desirability for complete freedom in basic research, whoever may be the sponsor.

Frankly, I do not believe that such a situation could long continue without there developing among the scientists a great deal of personal concern at the lack of relationship with other technical activities in the company and at the apparent lack of vital interest of the company in their results. Such a situation is

hardly analogous to university research, where the scientist feels no obligation other than the accepted goal of good scientific work and good leadership for his students. I believe that company scientists cannot be separated from an ultimate sense of responsibility for company well-being any more than academic scientists can disregard their personal identification with the teaching and research interests of academic organizations. I would, therefore, deduce that a key problem in regard to basic research in industry lies in the relationship of the scientist to the company, and that planning for research of this type calls for some concrete steps to make this relationship an effective one.

In addition, we have the problem of selection of fields in which to perform basic research. Presumably, the scientists whom we select for such work should be free to choose their research interests and free to investigate in an atmosphere that is characterized by an absence of unfruitful inhibitions. It is not enough, however, merely to set the stage and then to leave all else to the intuition and independent judgment of the investigator. After all, the creative process is a realignment of relationships of known factors in new ways. How can we expect a scientist ignorant of company purposes, long-range objectives, and strategic capabilities to be able to point his work effectively in new fields of science? Certainly we can increase the chances in favor of significant discoveries potentially useful to the company by giving the scientist a better appreciation of future potentialities for company growth that might result from technical accomplishments in selected areas. The intuitive choices of a scientist in pursuit of unknown facts can be favorably influenced by his acquaintance with the long-range objectives of the company, as well as its capacity for development, its visualization of future marketing needs, and with the basic economics of the industries it serves.

It is not enough to say that planning for basic research involves only the problem of careful selection and adequate support of basic scientists. For industry, this would be an oversimplification. We must also face the problem of stimulation of the sci-

entist toward long-range company objectives through an enlightened exposure to the technical capabilities of the company and its long-range technical needs. In planning for basic research, industry must discard the concept of total isolation and seek ways to keep its exploratory researchers aware of other facets of company technical activities, encouraging close personal relationships with other technical people in diverse pursuits. Perhaps in cases where physical isolation is necessary, the scientists can keep in touch through periodic consultation with development personnel in other laboratories of the company.

Whatever the method, if proper means are taken to educate the company scientist on present and contemplated company objectives, it is more likely that basic research will flourish in industrial laboratories along lines selected by the research men themselves, as it should be, rather than by a superimposed plan, remote from the preoccupations of the scientist and therefore lacking the advantages of a flexible and intuitive approach to new factual revelations. Thus, I believe that educating our basic industrial scientists in the broadest terms of present and potential company interests, and keeping them in touch with technical people active in applied research and development work, is the best way to plan for basic and exploratory research in industry.

As a natural outcome of such a policy, we shall be much more secure in facing the inescapable and vexing problem of how much basic work to support in fields of present and potential company interest. In effect, we will be able to join the experience and practical background of research and development managers with the skills and intuition of the scientists. We shall not need to be arbitrary but can broadly share the responsibility for decision, with our most knowledgeable people working harmoniously together.

Applied Research

Out of successful basic industrial research will come certain leads or scientific breakthroughs that may provide a starting point

for development. However, before the inception of full-fledged development work there is usually a phase of work which involves a more careful definition of the nature of the lead or breakthrough in terms of more direct practical values. This is the phase of applied research.

Here again we have the problem of selection of scientists who can approach this kind of work creatively. In addition, there are other key problems in planning for applied research. Perhaps the predominant one is that which is imposed by the duality that is the inherent nature of business: To be successful a manufacturer must not only make products, but also sell them to customers. As soon as we start speaking about industrial research toward definite objectives, we must be thinking in terms of manufacturing and selling goods; in other words, we must deal with both the process and the use aspects. To debate the relative priorities of process research or applications research is fruitless. These aspects must be carried out in parallel, and the individuals responsible for each must be in constant communication. Planning for applied research must, therefore, provide for an interaction of the men who seek to define the best avenues for process development and those who seek to apply the product in the ways having the greatest commercial promise.

Historically, industrial chemistry has chiefly been concerned with the synthesis of products to meet predetermined specifications. Further, since many chemical products have fitted into the industry as replacements for much older products derived from naturally occurring materials, the habitual emphasis in industrial chemical research has been on the process aspects. In recent years, however, the need for an equivalent effort on applications research has become apparent, as we are discovering new materials which have inherent properties that differ substantially from the natural products they supplant. For example, in the application of new polymeric substances, recent commercial success and economic growth have depended as much on the creative skills of the applications research man as on the synthetic chemist. Thus the prob-

lem of balance between process and applications research in the applied research phase frequently reduces to the problem of finding adequate, effective, and sufficiently intensive approaches to the applications research aspect. This trend is well exemplified by the extensive facilities now provided by the leaders of the chemical industry for their applications research and development activities.

Development

As the likely outcome of successful basic and applied research, development inherits the same difficulties and involves the same problems in its successful planning as I have already described for the earlier phases—problems of selecting, stimulating, and supporting competent personnel and of maintaining a reasonable balance between process and use aspects of the work. However, the predominant problem of development becomes an economic one—a problem imposed by the fact that the successful project resulting from development must meet the criteria of commercial success. The central problem in planning for development is to establish economic criteria to guide the work and determine progress. These criteria can involve, in the simplest form, costs of manufacture and distribution and expected prices and volumes for sale to various markets.

Whereas basic research is apt to be an individual effort or the result of intimate cooperation of a few individuals, development generally involves a considerable number of participants tackling different parts of the same project. The team concept rapidly takes over, and yet the problem of maintaining individual responsibilities and creativity is also of importance.

Because of the increasing costs and complexities of administration of the project, coupled with the requirements for economic evaluation, the course of development must be laid out in advance and an attempt made to maintain time schedules and predetermined budgets for this type of work. However, any such plan must remain flexible, for a too rigid system of advanced

planning can stultify individual effort—to the detriment of effective results.

Meeting these two kinds of problems, the problem of predetermined economic goals and the problem of effective teamwork, does not involve any insurmountable difficulties. With adequate forethought and preparation, a well-rounded development can be a very exciting and satisfying experience for all of the technical participants.

The necessary forethought as to economic criteria requires a spelling out in advance of process costs, coupled with investment estimates, based on the results of applied research. The cost estimator working in this phase is not to be thought of as one who imposes inflexible rules on the game. Actually, he is the one who may be able to put his finger on the points of greatest difficulty which must have good technical solutions if the project is to survive. An economic estimate at this stage may be little more than a statement of hope, but it can also provide guidance as to how that hope may best be realized. Too often the economic factor is apt to be regarded as an inhibiting influence rather than what it should be—a helpful guide and stimulant to the technical men engaged on the development project.

In parallel with the cost aspect, there must be equivalent forethought placed on the use and market aspects. If applied research has generated definitions of usefulness of the product, these can be matched to the known market possibilities and to the unfulfilled needs of potential users in an imaginative fashion. On this basis, a market potential, general or particular, can be evolved which serves as a guide and stimulant to applications development in much the same way as does the cost estimate in its influence on process development.

Again we have the problem of balance between process development and market development work, with the latter too frequently taking the back seat. This discrepancy is gradually being remedied, for with the greater dependence of mankind on

synthetic materials, the importance of adapting chemical products to fulfillment of these needs is becoming clearer.

Successful planning for development is increasingly dependent on finding a satisfactory solution to the problem of formulating the applications and market development program. Frequently, we encounter an associated problem in the selection of suitably qualified personnel. The universities give little or no training today in applications research and development. As a consequence, the people selected for such work must be trained by association with others of longer experience. The knack of visualizing and accomplishing effective applications work is a special one. However, the enthusiasms for such work are infectious, and the achievement of one successful result promotes further successes. The grouping of such people together helps to overcome the strong natural inclination of university-bred research men toward work of a synthetic or analytical nature.

The addition of personnel having strong basic research training into such groups also tends to overcome the empiricism inherent in the first approaches of applications work. After all, the principal reason why applications work may often be routine and uninspired is the lack of basic knowledge underlying the simple practical behavior of material things. Elucidation of the basic factors can go far towards rationalizing such behavior and putting applications work on a higher scientific plane.

After adequate preparation has been made for setting up economic criteria and organizing the technical team, development planners will be concerned with the problem of continuing project strategy—the need for timely decisions on intensity of effort, concentration on special difficulties, balance between process and market aspects, meeting business deadlines, and the like. How are we to identify the decision points and be sure to bring the business managers into this action at the right time? At Hercules, we subdivide development into three phases, laboratory, design, and commercial, and set up specific criteria for advancing a project into the next phase. Periodically, we also bring together a review

group including the operating department manager, his development director, a company vice president, the laboratory director, the research division manager, and the director of research. Each of these six men has a portion of the responsibility involved in the project. Their periodic discussions can bring about harmony in views and clarify future strategy.

As early as feasible in a development project, plans must be made for extension of applications work to the field. Up to that time, the information on potential use must come either from general knowledge of customer needs or from market research studies defining potential requirements for products displaying utility of a particular kind. The problem of determining product marketability changes from one of prediction to one of evaluation. As soon as sufficient material becomes available, field tests are generally arranged. In fact, sufficiently large samples may be prepared by laboratory methods to provide for initial field evaluations. Subsequent planning of larger-scale development phases may be seriously affected if reliable correlations of laboratory and field evaluations cannot be established very early in a development.

Parallel with this problem of linkage of applications study inside and outside the company is the problem of linkage of process work between laboratory and pilot plant and the engineering and operating organization in anticipation of the commercialization of the project. Satisfactory planning at the advanced development phase must anticipate the difficulties involved in this transition and provide for adequate overlap in responsibilities of the various company units involved. As a specific example of a typical difficulty: Cost estimates prepared by development men, construction engineers, and plant production people may differ in their degrees of optimism. It is best to have a meeting of minds before the time of final decisions!

Both of these linkages of the project—potential product consumption, on the one hand, and plant operation on the other—are aspects of the central problem of commercialization: the

transition from the idealized conditions of research and development to the practical conditions of the real world. Planning for research must provide for a courageous but sensible approach to that problem. As a suggestion to others who wish to engage in planning for research: The sooner you get operating management in the boat with you, the more likely is this transition not to be painful; or if it is, the more people there are to commiserate and find another way out of the difficulty!

In summary, the major problems involved in planning for research stem from the transitional nature of industrial research. We must take account of extreme contrasts, ranging from highly individual pursuit of basic objectives, to carefully organized teamwork needed for advanced development. Planning must be flexible and changing as we deal with different phases of the research and development spectrum. The scientist must be aided, through education on long-range industrial objectives, to accept greater responsibility in choice of fields and emphasis for basic research. In applied research, we must define targets relating both to manufacture and to utilization of products, effecting a good balance between process and applications research. In development, we must map progress in terms of tangible commercial objectives and economic measures of progress. In all planning for research, we must not attempt to dispel apparent conflicts between scientific and commercial factors. On the contrary, we must seek to evolve ways to keep our planners cognizant of the importance of both of these aspects, and skilled in interweaving them into new patterns of industrial research.

DEVELOPING MANAGERS OUT OF CREATIVE SPECIALISTS

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Introduction

Research management is concerned with the problem of developing supervisors and managers out of creative specialists for two reasons: (*1*) Future executives will increasingly be chosen from the ranks of creative specialists in research, development, marketing, and other new line and staff functions; and (*2*) Developing supervisors and managers out of creative specialists is often more difficult than development of managerial personnel from those engaged in the older activities such as manufacturing and finance.

With respect to executive recruitment, the general problem is that an expanding economy continues to put pressure on the resources of managerial talent. It also breeds more specialization. As the newer functions like research become more important and even vital to many companies, the need for personnel to fill middle

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and top management positions increases. Hence, in addition to preparing creative specialists to fill supervisory and managerial posts in their own functional area, we are also faced with the problem of developing functional specialists into managerial generalists for the company at large.

But there is reason to believe that the traditional management development techniques, used with moderate success in the older functions, may be quite inadequate in the newer functions of research and marketing. The essential difference between the older and newer functions is the *predictability* of action of the former. By contrast, research activity is relatively unpredictable. Unpredictable activity is difficult to manage, and difficulty of management makes for difficulty in training managers.

The consequences of failure to adequately develop managers become more serious in an age of specialization. Reassignment at the same level is difficult because of the specialist's background, which delimits areas of assignment. Managerial failures must, therefore, be down-graded or "kicked upstairs" to get them out of the way. If retained they must be surrounded by a costly, supporting retinue to make up for their deficiencies.

I want to review briefly some traditional techniques of management development, note their inadequacies for the task at hand, and explore a more basic and practical approach to developing creative specialists into supervisors and managers. The problem of *selection* of managerial candidates lies outside the scope of this article.

Traditional Management Development Techniques

A brief review of traditional management development techniques may be useful because, as they are valid for one function, they may have some validity for another. This follows from the assumption that the management of men has some common denominators no matter what the specific activity may be. Emphases may vary, but the concerns are the same. All work done within

the confines of any organization must be planned, organized, executed, and appraised. Knowledge of the deficiencies in the traditional techniques may point the way to a resolution of the problem of converting creative specialists into supervisors and managers.

The management development techniques designed to strengthen and exploit the managerial potential of promising candidates include job rotation, broadening mental horizons through the study of the humanities, and the establishment of a dual management of specialists and administrators.

The one certain effect of job rotation is that it acquaints the manager or managerial candidate with the various activities or functions with which he may someday be concerned in a position of greater responsibility. It may do no more than that. It is, so to speak, company-oriented. By contrast, the humanities approach is individual-oriented. The individual is to be developed into a better, more understanding person. The third technique, dual management, is a compromise. It grew out of a sense of incompatibility between the management of technical and business aspects of research and development activities. The rationale appears to be that management of technical and business activities have little or nothing in common and therefore should be separated.

The basic assumption that seems to underlie these methods of developing supervisors and managers is that interest, broad knowledge, and innate ability determine whether or not individuals can climb the managerial ladder. Hence, if they have managerial potential, they need only be introduced to whatever has to be done or known by managers. Or, if technical personnel do not have an interest in and knowledge of business practices, one keeps them away from the managerial and encourages them to climb the technical ladder.

An interesting point about these traditional techniques of management development is that they are devised in terms of the *deficiencies of specialization* rather than the *nature of management*. That is, since the specialist knows only what he is doing,

the assumption is that if he finds out what others are doing and/or learns to be tolerant of other people's idiosyncracies, he will become a manager. The specialist is to be fortified and supplemented as if he were deficient in vitamins. In the case of persons destined for the technical ladder, the assumption is that the individual is not merely deficient in but practically devoid of managerial talent as defined in business circles.

Management, however, is not the manifestation of instinctive behavior, but the expression of administrative skills which have been learned. As a creative process, it includes the basic concepts of planning, organizing, executing, and evaluating work done by a formal grouping of people. Management methods or principles do not depend on the specific activity being managed, although *emphases* will vary, depending on the function being managed. For example, a research worker needs more motivation and less control than a production line worker, and the division of labor cannot be delineated as sharply in the laboratory as in the factory.

Overall, the *process* necessary to effect the *management of an activity* must be distinguished from the activity per se. Essentially, this becomes a division of labor between the *execution of the work* and the *supervision of people* who do the work. It is the failure to recognize this distinction when using management development techniques which often inhibits success in their use.

The Role of the Specialty Supervisor

Let us discuss the distinction between worker and supervisor. The supervisor is distinguished from the worker in that the latter is entirely engaged in operations, while the supervisor is concerned mainly with supervision—either workers or other supervisors—although he may have some operating responsibilities too. For example, the classical type of first-line supervisor, the foreman, is primarily concerned with workers as the *means* by which the operations—or *ends*—are achieved. The foreman does not routinely involve himself directly in operations the workers perform. For

example, the foreman and the worker cannot both operate the same drill press. If the foreman takes over for a while, it is only to teach the worker or to relieve him temporarily. Hence in production-type operations, the very nature of the work forces a distinction between execution and supervision. The foreman achieved his status partly to get away from the drudgery of manual labor. He encourages his men to operate the drill presses, load the freight cars, or tighten the screws on the left side of the assemblies. He oversees the work to assure the production of so many units of work, meeting certain prescribed standards per unit of time.

This picture is in sharp contrast to the situation in which we find the first-line supervisor of a creative specialty. The creative-specialty supervisor may be a physicist directing the work of a research team, an engineer administering a small engineering section, or a copy supervisor in charge of a half-dozen copywriters. The difference between such a supervisor and a foreman on the production line is that the intellectual content of the work interests the specialty supervisor. For this reason the specialty supervisor cannot so easily disentangle himself from the work his men do.

While the foreman and a worker cannot both man the same drill press, both the specialist supervisor and his subordinates can concern themselves with the development of a new technical process, the design of a new product, or the details of a new promotion plan. In these situations, *doing* the work and *supervising* the people who do the work blend together. This is understandable, for supervision of a specialty operation often involves highly subjective standards by comparison with production and assembly line operations, where specifications are clearly and objectively set forth in blueprints and quality control standards. The specialist supervisor has to immerse himself in the work of his subordinates, for how can he judge their work if he isn't directly familiar with it? As suggested above, it is difficult if not impossible to set up objective standards for intellectual specialty work which can be enforced by quality control inspectors, as in production of standardized material goods. The supervisor of a creative specialty opera-

tion is the quality control inspector, as well as colleague, consultant, friend, and superior of the worker. As a consequence, the first-line specialty supervisor tends to work *with* rather than through people.

Unlike the foreman, in short, the specialty supervisor does not have many clear-cut opportunities of engaging in supervisory or managerial activity, because he is not able to divorce himself from direct participation in the work. Occasionally, a specialty supervisor is promoted *precisely* because he is a good specialist, rather than because he displays signs of managerial potential. The frequency of such promotions is shown by the common question, should you make your best salesman a manager?

It is not difficult to visualize the situation in which a good specialist becomes a poor, that is, inadequate, first-line supervisor. On a Friday afternoon our specialist is informed that he is promoted to replace his supervisor who has been promoted. On Monday morning he takes over supervision of the team or organizational unit of which he was a member. His interest in the work continues. Occasionally he will have to engage in some supervisory chores: recommend a raise for a former colleague, and appraise another because the company has a personnel appraisal program, attend a sectional or departmental staff meeting, and the like. But his heart really beats faster when he is at the lab bench or the drawing board with the other fellows. If he is a reasonably sociable and intelligent person, this specialty supervisor to all appearances can succeed in his job. And he may be promoted a second time because of the fine results he has produced as a *working* supervisor, rather than because he has managerial ability or even potential.

However, the situation at the second level of supervision is different from that at the first. Now there is a line of specialty supervisors between our newly promoted manager and the specialty workers. Since he is accustomed to working directly with personnel at the lab bench or drawing board, our second-line manager may continue to do so, treating first-line supervisors

simply as especially skilled workers. Isn't this inevitable, since that's what our new second-line manager is himself—still? Not having learned to distinguish between his supervisory and operating tasks when he was a first-line supervisor, he is not able to detach himself from the details of his responsibilities, even when he is two echelons away from the work. He is unable to delegate authority to, and exact responsibility from, the first-line supervisors, reserving for himself the task of coordinating and directing their activities overall. He fails to become a manager because he is unable to leave his specialty baggage behind as he rises. With such a history, job rotation, individual broadening in humanities, even dual management, alleviate but do not eliminate the basic difficulty: failure to learn and engage in managerial responsibilities.

Resolution of the Problem

This analysis of the basic nature of supervisory and management development suggests that such development is an on-the-job process basically, rather than a technique applied in special doses after hours or away from the job. If the analysis is correct, remedial—or preferably, preventive—measures are not difficult to establish. A three-stage program, is involved: *orientation, guidance,* and *periodic appraisal. Orientation* should be undertaken by the new supervisor and his superior immediately. It consists of carefully distinguishing between the supervisory and operating duties of the job. The supervisory tasks include the two broad categories of (*1*) maintaining the organization and (*2*) planning, supervising the execution of, and evaluating the work.

The aspects of maintaining the organization consist of *manning:* hiring, firing, motivating, appraising, and promoting personnel; *budgeting:* planning the monetary requirements of the organization; *provisioning:* providing work space, furniture, equipment, machinery, tools, supplies, and other resources; and *establishing standard operating procedures:* methods of doing rou-

tine work, standards of performance, channels of communications, content and periodicity of reports, etc.

Planning involves the formulation, interpretation, and communication of the organization's objectives. It includes the supervisor's relationships with his subordinates as well as his superiors. Two-way communication in planning is especially essential so that the general objectives set by higher management can be given specific, valid content by subordinate personnel.

Directing the execution of the work includes initiation of new activities, guidance and consultation, motivation, and pressure toward completion of the work. It is in this activity that many first-line supervisors confuse their roles and immerse themselves in the work instead of supervising the workers.

Evaluating the work involves comparing achievements with the original goals, not only in terms of the end product but the processes used and the resources expended, to inculcate a sense of efficiency and to establish benchmarks for future planning.

The purpose of the review of the supervisory responsibilities of the new supervisor is to assure initiation and expansion of such activities. By contrast, the examination of the supervisor's operational responsibilities is done for the purpose of *limiting* such activities to those that are essential.

If the individual in question is a "working supervisor" because of the small size of the organizational group, his areas of work and the amount of time he is to spend at such work should be considered and delineated. The new supervisor will also have operational responsibilities which stem from his new position. These will include attendance at staff meetings, committees, and coordinating conferences, direct liaison with specific areas of the organization whose work affects, or has potential for affecting his own responsibilities, special tasks delegated the supervisor by his superior, and the like.

When the orientation stage has been satisfactorily completed, the supervisor's superior should begin the stage of *guidance*. He has to exercise caution in this temporary but close relationship.

It should be an ear-to-the-ground activity rather than a manipulative control of the new supervisor. The latter must be the medium through which the superior keeps in touch with the supervisor's responsibilities; he should not retain or establish new contacts with the operating personnel. By discussing the new supervisor's activities candidly with him, especially by inviting him to share his new problems and offering sympathetic and helpful—rather than critical—advice, the superior can keep in touch with the situation and help the supervisor through the initial adjustment to his new job. The nature and frequency of these close superior–supervisor contacts depends, naturally, on the specific situation. Too much attention is almost as bad as none. Generally, though, superiors probably err in devoting little or no attention to developing their first-line supervisors. Their attitude usually can be summed up in the phrase "sink or swim."

Oddly enough, few supervisors would think of putting a new person to work in operations without a period of training. But when it comes to supervision, the assumption seems to be that a person is, or isn't, a born manager. Hence, the new supervisor undergoes trial by ordeal—if he swims or even floats, he is supervisory material. If he sinks, the next man takes his place. A medieval technique for detecting witches is not suitable for selecting modern supervisors.

The third stage in developing supervisors, *periodic appraisal,* is one that should never end. Although especially important in the immediate post-appointment period, the appraisal function must be continuous. This is particularly so in creative work, where the activities are likely to change from time to time, and, therefore, may require different supervisory emphases. Unless appraisals are made periodically, supervisory behavior adequate for one kind of work or at a particular time will become standard behavior for all kinds of situations through sheer habit—with disastrous results.

The new supervisor should be formally appraised within a month or two, certainly not more than three, to interpret and rein-

force the guidance the supervisor has been providing on a continuing basis. The emphasis in the appraisal should be "how are *we* doing?," not simply "how are *you* doing?" In stressing the group, the superior, the supervisor, and the worker's attention is properly focused on the total human situation. It is in this context that the supervisor actually works, and, therefore, the context in which he should be appraised. Such an appraisal need not, and should not, shift attention from the *supervisor's* performance, but instead, put it into the right perspective.

The formal appraisal should accomplish what the day-to-day guidance cannot do: summarize the developmental process to date. Progress in the understanding and use of supervisory tools and techniques and achievement of organizational objectives through intelligent planning, sensible direction, and honest evaluation of effort should be reviewed in a spirit of mutual confidence.

Throughout the appraisal, emphasis should be placed on the distinction between managerial and operational activities to reinforce understanding of the differences in responsibilities. When the superior senses that the supervisor not only understands intellectually, but has assimilated the understanding into his behavior pattern, then the superior should end the special relationship of guidance and rely on the normal communications pattern for information about the supervisor's activities.

Although direct guidance is terminated, the frequent appraisals should be continued. The supervisor should be formally appraised two or three times more during the year after close guidance is ended, to continue reinforcing his understanding and appreciation of his supervisory responsibilities. Thereafter, once a year appraisal will probably be adequate.

All of this may appear to be an unusually heavy program of supervisory development, time consuming, and perhaps even tedious. It must be balanced, however, against the consequences for the company, the superior, and the supervisor's subordinates, if the supervisor fails to make the grade. Of course, not all first line supervisors require the same amount of training. Some are

born managers, while other will just barely become proficient. We can assume, though, that the managerial potential in eligible candidates probably follows a bell-shaped curve. Between the two extremes lies a large percentage of persons whose potential needs—and will repay—cultivating.

The Need for Continual Development

It is easy to confuse the overall work accomplishment of a specialist supervisor with supervisory achievement. The lack of a clear-cut routine, the absence of standards of measurement, the high ability of the workers, these and other factors obscure the activities of a specialist supervisor *as a supervisor*. If the first-line specialist supervisor has not received adequate guidance in supervisory behavior, he will find that promotion to the second-line supervisory position puts him in an untenable position. Now he no longer has direct access to the lab bench or the drawing board. His primary task, accomplishing work through other supervisors, may seem strange to him. Instead of managing he will meddle.

Not that the average second-line manager is necessarily an accomplished executive because he has been trained as a first-line supervisor. The interests, duties, and supervisory techniques appropriate to each level of management are different. For example, at the first level of supervision, the supervisor is equally interested in the workers and the operations in which they are engaged. He is intimately acquainted with both on a day-to-day basis. The second-line manager, by contrast, is less concerned with the workers and the operations and more concerned with the results his supervisors are getting. In each successive level of the management hierarchy, the manager is further removed from the locus of operations. He exchanges his knowledge and personal contacts in *depth* for increased knowledge and contacts in *breadth*. He delegates and coordinates more and operates less. The manager must avoid involvement in operational activities, while engaging more and more in managerial activities.

Since the manager's job changes with each promotion, it follows that his training in management never ends. With each job change he must be oriented, guided, and appraised until he has clearly demonstrated his capability in his new job. Management development techniques such as job rotation and special studies have a place in the training of managers, but they can only supplement and reinforce the basic responsibility of managers themselves to train their subordinate supervisors.

Much of the foregoing assumes that the supervisor's superior is an adequate manager himself, able to coach his subordinates in the managerial skills he himself commands. If he is not, the bad examples he sets for his subordinates will probably offset all the developmental assistance the subordinates can get from special training courses, job rotation, and other developmental devices. In such situations, only born administrators can develop in the right direction. This is too bad, for in view of the continued demand for managerial talent, the majority of managers will have to be made, not born.

Summary and Conclusion

The development of supervisors and managers out of creative specialists is becoming a more crucial problem because (*1*) companies which increasingly depend for survival on research and development and marketing recruit managers from these newer functional specialties, and (*2*) it is more difficult to develop supervisors and managers out of creative specialists.

The reason for this difficulty is that in creative, as opposed to routine work, it is not easy to distinguish between supervision and execution of work. Besides, creative work continues to provide intellectual interest for the specialty, first-line supervisor. The traditional management-development techniques, used with moderate success in the older functions like manufacturing and finance for which they were designed, are inadequate by themselves for developing supervisors and managers out of creative specialists.

To initiate a new supervisor into his responsibilities, a three-stage program of *orientation, guidance,* and *appraisal* should be prepared and carried out. The orientation stage is designed to acquaint the supervisor with his supervisory and operating responsibilities; the guidance stage will enable the superior to assist the supervisor in learning to execute his supervisory responsibilities; and the appraisals will allow the supervisor's progress to be reviewed formally for maximum reinforcement. Since managerial duties change as one progresses up the managerial ladder, management development must be a continuous process.

The traditional management development devices can be used to supplement the basic process of on-the-job development, but they are not an adequate substitute. Perhaps the uneven results secured with the traditional techniques are due to their use as substitutes rather than supplements in the training of supervisors and managers.

PROCEEDINGS OF INDUSTRIAL RESEARCH INSTITUTE STUDY GROUP MEETINGS. NUMBER 4. OPTIMIZING THE RELATIONSHIP BETWEEN RESEARCH AND MARKETING*

STAFF REPORT

Introduction

Two conferences on that topic were held in 1960, one in New York, N. Y. on February 9–10, the other in St. Louis, Mo., on March 8–9. Seventeen persons attended the first meeting, sixteen were present at the other. On both occasions, the discussion leaders were Dr. Lyle I. Gilbertson, Administrative Director, Central Research Laboratories, Air Reduction Company, Inc., and Mr. Erwin G. Somogyi, Vice President, General Manager, Research and Engineering Division, Monsanto Chemical Company.

Industrial Research and Marketing

Since industrial research is conducted primarily for economic considerations, there is a need for guidance of the technical effort

* The circumstances which led to the formation of the Research Management Study Group Conferences and their objectives are covered by Vaughn in *Research Management,* III, No. 2, 93 (1960).

toward the areas which appear most likely to yield results of greatest advantage to the company. Such guidance is a line function of management in the exercise of which, management utilizes the assistance of the staff marketing organization. The duties of the latter include the collection of pertinent data, analysis of their significance in current operations, probable impact upon the future, and the formulation of recommendations helpful to Management in the decisions it makes, which in turn influence the nature and direction of the research effort. Thus is the work of management, research, and marketing interwoven into the fabric of company activity. The achievement of an optimum relationship between Research and Marketing within policies set by the company management is of obvious importance.

Both activities have objectives that are similar. Both operate in dynamic fields of technology; fields that are heavily weighted with uncertain factors whose import cannot be fully assessed. Each activity requires its own competence and training; each has developed and is improving its own techniques for proficiency of operation. While full cooperation between the two is necessary, this is not easily achieved, for the type of work performed by Research differs greatly in nature from that of Marketing. Personnel in each field differ substantially in their training, their methods of procedure, and in the environment in which they operate. Therefore, conscious effort and planning are needed for coordination of their effort to avoid difficulties which otherwise are sure to arise.

An Example of Misunderstanding

A participant at one of the study groups described an important project of his company which culminated in a new drug. Two decades of effort in the research laboratory were expended before achievement of the breakthrough which permitted the development and production of the product. During that time, the marketing organization of the company had repeatedly investigated

the need for the drug, and each time had concluded that the potential was smaller than the investment required, and had repeatedly recommended that the project be dropped. Research work was none the less continued, and when the product finally reached the market, it was discovered that the demand far exceeded all estimates, and the item became one of the most successful in the whole company line.

The incident provides an illustration of two of the causes of misunderstanding between the marketing and research organizations of a company: marketing estimate which proves to be utterly wrong; the difference in the perspective of research as contrasted with marketing. The former sometimes tends to work toward long-range goals, which when realized will assure great future growth, while marketing generally favors shorter-range projects with quick payoff.

During the conference at which the incident was described, the speaker introduced a concept of "use potential" as distinguished from "market potential." The latter can be determined with reasonable accuracy with respect to products similar to those presently in distribution. However, the estimation of demand for an item which is quite new—one which involves a new Use Potential—is in most cases a practical impossibility; the factors involved being too uncertain to estimate. A mutual understanding of this limitation of market research removes one of the stumbling blocks to effective cooperation. Another step toward good understanding is recognition of the fact that the decisions which the marketing organization is required to make are fully as difficult as those which are in the province of Research, and the decisions in both cases are of vital importance to the company.

Functions and Interrelationships

One of the corporate objectives which is of immediate concern to both the research and marketing activities, is the protection of the company investment. Such functions as customer

service, product and process development, evaluation of current trends in technology and trade, and other activities are designed to maintain or increase the market for a given product. This type of work safeguards the competitive position and insures satisfactory return on investment. It can be aided by intelligence data. Intelligence provides useful clues with regard to the best selection of projects for research and development work. It is an aspect of business operation, to which alert personnel throughout the organization can contribute. For example, the salesmen of a company can play an important role. Their activity involves contacts with prospective and regular customers during which they can learn about customers' difficulties, obtain their opinions of existing products, determine their unfilled needs, learn about currents trends, and about the activities of competitors.

Such data can be productive of valuable intelligence information, and some companies have well-established channels, generally under the cognizance of the marketing organization, for the collection, sorting, and evaluation of all such data. The information, suitably analyzed and evaluated by marketing personnel, is prepared in the form of reports which are forwarded to Research for guidance.

The principal function of the salesman is, of course, to sell the company products. He generally has a quota toward which to strive. Intelligence work is a sideline, but one that is important enough to justify a system giving appropriate recognition to those men in sales and other departments who perform a service beyond their primary duty in order to provide information of value.

An important element in the protection of a company's market is the performance of development work on existing products. The objective may be the lowering of cost with maintenance or improvement in quality, the discovery of novel properties which can be publicized to aid the sales effort, or the discovery of new uses to broaden the market base. This type of investigation is of a continuing nature. It goes on during the life of the product, and is highly necessary, if for no other reason than to keep up

with competition. From that standpoint, the work may be regarded as a defensive action. It does, however, also have offensive aspects in that extraordinary results may lead to such improvement as to enable the capture of a greater share of the market.

In general, however, development work on existing products or processes may be regarded as short-range. The problems which have been brought to the attention of research by the marketing organization have in all likelihood also been made known to competing organizations by their own people. Several development laboratories will thus be working on the same or substantially similar projects. Under the circumstances, it is not probable that the results of the research will be unique and capable of forming a new base for unique corporate growth.

Companies that have multiplied their sales and become leaders in their fields, have more often made their achievements through long-range programs which had their beginnings in fundamental or exploratory research; the type of activity that is based upon new and original ideas, not merely modifications of the old. That is the work which is most likely to provide a foundation for products that are new or vastly superior to those in current use and for the creation of entirely new industries.

In striving for the original, many companies have one or more sections of their research organization staffed by personnel who devote themselves to basic, fundamental or exploratory research. These sections are usually removed from the day-to-day work of the company because their mission is to provide the technology for long-range growth and the results of their efforts in terms of new fields of investment are long in coming, ofttimes discouragingly long. In contrast, the applied research and development laboratories of the same company work on projects which often terminate in a relatively short time with results of immediate value. For that reason there is sometimes a tendency to regard the latter activity as one of greater importance.

There is another factor which operates against a well-recognized and stable balance between applied and fundamental re-

search. The short-range problems awaiting solution are generally more numerous than the available scientific manpower. Furthermore, these problems affect the current business of the organization, and this motivates an urge, particularly on the part of marketing and sales personnel, toward utilization of additional scientists —available, for example, in the basic research section—for the timely completion of projects which otherwise must wait their turn. Instances can in fact be cited where scientists were taken from their long-range work and assigned to projects of more immediate utility. In most cases, the transient advantage gained was more than offset by the detrimental effects upon the broader interests of the company.

The stability of operation of a basic research section depends upon the support of a management determined to resist the occassional strong temptations to make reassignments of its scientific personnel to short-range projects of quick payoff. The marketing organization, which also has long-range planning functions, can be convinced of the wisdom of this policy which is part of the general objective toward better understanding.

Means for Achievement of Coordinated Effort

One means is to have the research organization offer at suitable intervals a preview of scientific developments in the formative stages. The occasion might be an "open house" to which the marketing and sales people are invited. They can witness demonstrations of the potential value of research projects underway, and discuss the implications with those responsible for the work. Results of the program are mutually beneficial, for the marketing people become aware of developments which may eventually require their services; and their specialized knowledge may lead to suggestions as to the specific lines along which the technical effort should proceed for commercial success.

The "open house" or similar program of display of research work accomplished and in progress is an effective means of

acquainting salesmen with the capabilities of new products. The salesmen develop more confidence and speak with greater assurance about company products when they have learned at first hand what such products can do and which industries might be interested in using them. One participant, for example, described a demonstration in a pharmaceutical laboratory. It involved an obstreperous monkey who, subjected to the action of a new tranquilizer, showed a change in demeanor which impressed all the witnesses.

In addition to the tangible results which follow an exhibition of the results of research work, such meetings are promoters of mutual understanding, cordial relations, and effective teamwork.

One objection to the display of results on projects still in the exploratory phases is that such work is often regarded as having confidential status. However, if the existence of that status is made known to others in the organization, experience has been that the confidence is respected.

Some companies have mobile displays which are moved to designated units of the organization to acquaint people with the R and D work accomplished and in progress. This kind of exhibition has its advantages in that less travel of personnel is involved, and there is less interference with regular operations. However, a mobile show may not be as elaborate as one held in the laboratory, nor does it provide as good an opportunity for discussions at first hand with the scientists involved.

Technical and trade meetings that are of interest to a company are quite generally attended by an appropriate representative. One person observed that such meetings can be of greater value through joint attendance of persons from Research and Marketing because the subjects of discussion can be considered in their scientific and commercial aspects. Joint attendance may lead to better application of the potential in new developments which are introduced at such meetings and may also be a means of promoting teamwork.

In the course of their work, salesmen are occasionally con-

fronted with a technical problem on which the customer needs help. The problem may require study at first hand by a member of the R and D department. But the matter of having a research man visit a customer is sometimes a delicate one, involving as it does a possible feeling of encroachment of one department upon the province of another. Where relations are good, that consideration does not arise and a well-functioning organization frequently has requests from Sales for a member of the R and D staff to go into the field and give technical assistance as a part of the program of customer service.

Selection of Projects

The selection of projects for research and development work generally follows conferences among representatives of the research, marketing, and sales organizations. Proposals are discussed, priorities established, and differences are ironed out in so far as possible. In practice, it appears that the ideas for most projects originate with the personnel in research. However, the other departments do contribute a part of the total program, and in consumer-oriented companies, the proportion can be large. Several persons reported that the method of setting up a research program which gives the most effective results, is one which makes provisions for the exchange of views among research, marketing, and other personnel concerned. There is a natural tendency for each segment of a company to favor its own opinions on what constitute the best projects for research. A full and frank discussion in which the relative merit of each project is estimated and a proper screening made, commensurate with the resources of the organization, is the most satisfactory means of arriving at decisions. The annual program of research, which is finally submitted for management approval, is thus a resultant of discussions and agreements reached at informal conferences, committee meetings, and formal meetings of personnel representing the departments concerned.

In contrast to the more applied fields, the selection of projects in fundamental or exploratory areas is generally the responsibility of the research director, who makes his decisions within such bounds as may be set by company policy. The marketing organization commences an increasingly active participation as a project advances from the exploratory stage of research to the applied, development, and pilot-plant phases. The increase in attention by the more commercially oriented part of the company is necessary for the reason that as the R and D spectrum is traversed, larger-scale operations become necessary. Costs rise sharply, and it becomes imperative to recognize and delineate as closely as possible those segments within the fundamental research areas which show the greatest promise of success from both technical and marketing standpoints. Furthermore, the developments which approach the phase of commercial exploitation require planning in respect to production, introduction of the product, sales, distribution, technical and customer service. All of these aspects are of concern to the marketing organization.

One of the participants described the procedure in his company for the authorization of a developmental project. A form is used to record the objective of the investigation, the probable cost, and the date of completion. An estimate of the probability of success is made. Other entries on the form deal with the commercial phases. The market potential of the proposed innovation, whether it be a product or process, must be estimated. The proposed distribution channels, anticipated manufacturing cost, and selling price are recorded. Relevant data in regard to competitive products are noted. The form provides for the signatures of the product manager and the managers of development, market research, manufacturing, and sales. Space is provided also for the approvals of the vice presidents of marketing, manufacturing, and operations. The entire procedure has, in fact, built-in features to assure appropriate study of a proposal in all its aspects, and this provides greater assurance of interest and co-ordination of effort

among the departments concerned, before formal authorization is given to begin the development step.

The procedure outlined above supplies the mechanism for co-ordination of effort. When, in addition to the formal procedure, there is present also a spirit of understanding, trust, and mutual respect between the research and marketing organizations, the conditions for real progress are achieved. One participant observed that the directors exert an important influence in setting the pattern of good relationship, for if they work well together, the other people tend to follow the example.

A company in a consumer-products industry has to be particularly sensitive to innovations in fashions and taste, for these elements have a profound effect on the success of a product. Such an organization makes every effort to be geared to quick recognition and response to change. To this end, mechanisms have been developed for attainment of a high degree of co-ordination between research and marketing in the selection and review of research projects. The procedure in use in a large market-oriented consumer goods organization was outlined by a participant.

Problems which affect the business of the company are considered by a top management team that meets weekly. The team consists of the chairman of the board, the president, executive vice presidents, and the vice president for R and D. These men discuss the sales efforts, the activities of competitors, the progress made on projects underway, the need for new investigations, and all other matters of concern to current and future operations. The discussions culminate in decisions.

This weekly meeting is a formal one whose importance is indicated by the fact that each is several hours long. Each member of the team comes to a meeting equipped with the necessary information obtained at preparatory conferences that the member has had with personnel of his staff.

At the conclusion of each meeting, the decisions which have been made are communicated through well-developed channels to the personnel concerned. Since the company operations are

complex and varied, the channels of communication are extensive. They include, for example, conferences between the vice president for R and D and his managers. The latter receive general instructions in accordance with decisions reached at the meeting. They communicate the instructions to the appropriate section chiefs supplying such further detail as may be indicated and so on down the line to group leaders and bench workers. The managers hold periodic meetings with their people to consider the progress of work and decide on further action.

The marketing organization employs similar methods to make known to its personnel what is required of them and to review the progress of work that has been assigned. The people in merchandising and production are likewise informed.

Formal meetings in the R and D organization, attended by the vice president, provide the opportunity for presentation by bench workers of new developments arising from their work. Conferences between research and marketing groups are held to discuss matters of common concern.

This outline does not include other day-to-day means of communication among the departments of the company, and these are also very important. All of this effort is needed to cope with the problem of achieving co-ordinated action. In a large company, that problem is always complex, but particularly is this true of the dynamic consumer-products field.

The weekly meetings of top management do not by any means imply that projects are changed with that frequency. The projects are set up six months in advance by joint efforts of the research and merchandising groups. The meetings do signify that the work is subject to constant review in the light of conditions that prevail. When such review indicates that a project should be modified or dropped, this is done, and the reasons for the change are made known to the personnel involved in order to eliminate possible resentment. A new project is always available to replace one that has been discarded.

The above outline indicates that complex problems frequently

demand elaborate measures for proper resolution, but the governing principles may be stated simply: (*1*) decisions which affect the business are made at the top level; (*2*) the decisions, in suitable form, are communicated to the echelons concerned with implementation; and (*3*) the work is subject to constant review at the top level in order to adapt the efforts to the conditions that prevail.

WRITTEN COMMUNICATION

The informal, day-to-day interchange of views among personnel of marketing and research is supplemented by conferences of varying degree of formality at which discussions are followed by recommendations or decisions on matters of common concern. These means of communication are characteristic of a well-functioning organization. But, in addition to the oral interchanges, there must be written documents by means of which the problems, or results obtained by one department, are formally made known to the other.

The R and D organization, for example, may issue periodic research bulletins to appraise marketing and other elements of progress made. Another series of reports may be termed "technical service bulletins," which acquaint marketing and sales people with that aspect of the company's work. There may be product release statements to give data on new products or information on changes in existing items. Customer acceptance standards deal with specifications and methods of analysis or inspection. Product data books provide information needed by the salesman in his work.

It was pointed out that these formal reports of the research organization should have an optimum balance between information that is available and information that can be used in day-to-day operations. Data should be selective, otherwise they may be confusing. In that connection, a participant related an anecdote about a boy whose father was a prominent theoretical physicist. The boy asked his mother a question in physics to which she replied

"Why don't you ask your father, he knows so much more about it." "Yes," replied the boy, "but I don't want to hear that much about it!" Indeed, it is quite often the case that too much detail is more of a hindrance than a help, and the matter of deciding upon the quantity and variety of data useful to the salesman is one that merits the joint deliberation of marketing and research people.

The reverse channel of formal communication—marketing to research—may take the form of product manager reports or industry trend reports. These present information on the commercial aspects of company products or processes, possibilities for further company investments, unfilled needs of customers, trends in market preferences, activities of competitors, and other intelligence factors which have a bearing on current or future research programs. Formal reports may be prepared which present and analyze the significance of customer complaints for the guidance of research. All of these reports, whether originating in marketing or in research, are a means of imparting information supplementary to the informal exchanges and formal meetings, and all are necessary in the achievement of effective co-ordination of effort.

Communication between the market research and fundamental research organizations is not especially frequent, nor need it be. Marketing is not immediately concerned with the work because the nature of the final results is unknown to all. But a general inkling of the directions of the technical effort makes for good feeling and provides opportunities for discussions which can be useful even in the early formative stages.

Conclusion

The common areas of interest for both the marketing and research organizations include the protection and suitable return on the company's investment, development of the existing market, and discovery of new investment opportunities. Within these areas, the management policies, which are established to guide marketing and research, vary greatly in specificity. Some organi-

zations have well-defined objectives limiting the business, for example, to such fields of interest as communications equipment, baby products, products which can be made from designated raw materials, or items which can be sold in a particular industry. Other companies have objectives which are more flexible or even nebulous, being dependent upon market surveys, results of research, business conditions, and company resources. Management says in effect, "Bring your results to us—we will decide what to do with them."

As the objectives vary in their scope, so does the extent or degree of guidance given to marketing and research vary. But whatever the policy with respect to the two organizations, the welfare of a company, both in its short- and long-term aspects, is dependent to a great degree upon the effectiveness of the relationship between these two major functions.

STIMULATING CREATIVITY IN RESEARCH AND DEVELOPMENT

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While Zen Buddhism and "beatniks" are often related, there is much to be learned about creativity by reading books on Zen. Evidently more than one stimulus can be derived from it. In support of Zen as a stimulus to creativity, Dr. Suzuki,[1] an eminent contemporary Japanese exponent of that discipline, has said . . . . "But Zen is inflexible and would protest that the so-called commonsense way of looking at things is not final, and that the reason why we cannot attain to a thoroughgoing comprehension of the truth is due to our unreasonable adherence to a "logical" interpretation of things."

The advanced adherents of Zen practice an interesting exercise in creativity known as the koan. The Zen teacher asks a question that expresses a dilemma. To this the student must give an original answer not based on conventional logic but not demonstrably wrong. This is not easy and demands considerable originality or

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creativity. Consider, for example, this koan: "A sound is made by clapping two hands. What is the sound of one hand?"

The value of koans consists in teaching us that logic is not enough. This teaching is important in furthering creativity. In general, creativity results from the use of imagination, intuition, and chance or serendipity but not logic. After the initial creative step, logic or reason is valuable in verifying and developing the idea.

The first reaction of many young research scientists is to question attacks on the value of logic or reason in furthering creativity. However, let us consider what some very creative men have had to say on this subject. For instance, Max Planck[2] said, "Again and again the imaginary plan on which one attempts to build up an order breaks down and then we must try another. This imaginative vision and faith in the ultimate success are indispensable. The pure rationalist has no place here." Albert Einstein[3] adds, "There is no logical way to the discovery of these elemental laws. There is only the way of intuition." Long ago Francis Bacon[4] stated, "Men are beholden . . . generally to chance or anything else, [rather] than to logic, for the invention of arts and sciences." And more recently, Beveridge,[5] the famous pathologist, says, "The role of reason in research is not so much in the initial detection of new facts as in their verification, interpretation and development." Note how similar are these comments to those of Dr. Suzuki, quoted earlier.

Now people can certainly be trained to use logic. In fact, although scientists may never have a course in college called logic, each is indoctrinated with the logical system known as the "scientific method." Most of them absorb a sufficient knowledge of this method as undergraduates so that they can apply it after graduation.

But how can they be instructed in creativity, if it depends on imagination, intuition, and serendipity? Experience has taught that there is no one method that will work for all people, but the following observations are relevant to the problem: there are two

main types of research workers, the factual and the creative. Of course, most individuals are combinations of the two extremes. Everyone can be creative at times. However, in general, the factual people like to consider what is known and understood. The creative people are more interested in what is not known or understood. "Experts" in technical fields are mainly of the factual type. They can inhibit the creativity of others by destructive criticism of attempts at creativity. The good supervisor can further creativity by aiding the two classes of people to understand the contributions that each can make.

Studies of creative people have shown that most of them have broad interests in all fields of science. The "expert" tends to read and study intensively in his own narrow field. The creative individual reads broadly in many fields. This permits him to draw analogies and comparisons between relationships in his own and other fields. Creativity is stimulated by reading such publications as "Scientific American," "Nature," and "Science," as well as by reading intensively in one's own field. As Beveridge[5] states, "Variety stimulates freshness of outlook, where too constant study of a narrow field predisposes to dullness. Therefore, reading ought not to be confined to the problem under investigation nor even to one's own field of science."

The obvious and direct way of stimulating creativity is to reward and compliment the creative people in your organization. Actions that are rewarded tend to be repeated. If the people in your organization know that creativity is desired, and that creative individuals will receive recognition and rewards, many of them will strive to be creative. It is very unusual for anyone to be creative without trying.

Such recognition and reward aid in increasing the self-confidence of creative people and this, in itself, is important. To be truly creative requires great self-confidence, since it means that the individual proposing the new idea must believe in it in the face of all experience and tradition. The supervisor can aid by

encouraging his people to speculate and by complimenting them on their novel ideas, even when these are fruitless.

Sometimes, creative people become frustrated because factual supervisors or reviewers will insist that all speculations or theories be omitted from reports, talks, and technical papers. Only "facts" are to be given in these publications. This action is highly destructive to the morale of creative people and may also greatly retard scientific advance. Speculations, hypotheses, and theories, even when subsequently proved to be wrong, have time and again aided in the collection of much new knowledge. Many of the most unexpected advances have resulted from experiments designed to test an incorrect hypothesis.

Supervisors can aid creative people to realize that opposition to any new idea is to be expected and that such opposition, within limits, serves a good purpose. As Wilfred Trotter[6] has expressed it, "The mind likes a strange idea as little as the body likes a strange protein and resists it with similar energy. If we watch ourselves honestly, we shall often find that we have begun to argue against a new idea even before it has been completely stated."

Probably the most common method of stifling creativity is to assign creative people to routine, repetitive tasks. Creative people do not enjoy such tasks and generally do not perform well at them. The result is a highly inefficient use of precious manpower. Creative people will make many speculations, which are subsequently proven to be incorrect or incomplete. They must be protected from severe, destructive criticism. Creative people can be expected to have many failures. Factual people should have few. Nothing is more disastrous to real scientific progress than a pronounced fear of failure.

Beveridge[5] writes, "There is nothing reprehensible about making a mistake, provided it is detected in time and corrected. The scientist who is excessively cautious is not likely to make either errors or discoveries." He quotes Whitehead as saying, "Panic of error is the death of progress."

Supervisors should realize that there is something childlike

about creative people. The creative individual has not lost the innate curiosity and sense of wonder characteristic of the child. Factual people are more typically adult. Just as many adults crush and destroy the fresh outlook of their children, so factual supervisors can eliminate any tendency toward creativity on the part of their people. The supervisor who wishes to encourage creativity must give sympathetic attention to the speculations of his people. Then he must encourage them or others to test the new idea to establish its accuracy.

Probably the most important attribute a supervisor can have that will allow him to encourage his people to be creative is that of creativity. In other words, if the supervisor is creative himself, it will enable him to understand and encourage other creative people.

Since creativity consists in a new approach to a problem or a novel way of expressing a relationship, it is inherently a lonely occupation. It is to be expected that most people who learn of this new idea will attempt to disprove it. New ideas should be tested in this manner, since the chances are good that they are wrong. However, the creative person should be encouraged to pursue the new idea until it is definitely proved to be wrong. He is opposing the whole world, in effect, so he needs support. The supervisor must do his best to see that the new idea gets a fair evaluation before consigning it to limbo. Otherwise, only stepwise progress, one slight improvement after another, which can be achieved by logical means, is possible.

Many of the most original scientific discoveries were the result of chance. Of course, if the experimenter had not recognized the value of the novelty that resulted, there would have been no discovery. Most people have a tendency to forget or sweep under the rug any experimental result that cannot be readily explained by them. Great discoveries have resulted when the experimenter had sufficient courage to recognize that he has obtained an unexpected result and to investigate it further until he can explain it.

Creativity is difficult. But the rewards of creativity are so

great that supervisors must exert every effort to protect and encourage those who have the capability in this field.

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COMMENTARIES ON NEW BOOKS

The Two Cultures and the Scientific Revolution. C. P. Snow. Cambridge University Press, New York, N. Y. 1959. $1.75.

An ever-popular topic for discussion today, both at seminars and cocktail parties, is the widening gap between the scientist and technologist, on the one hand, and the humanist and layman on the other. Sir Charles P. Snow has neatly attacked the subject in this 1949 Rede Lecture at Cambridge University, later published as "The Two Cultures and the Scientific Revolution."

Sir Charles, defining his two cultures as that of the literary intellectual and that of the scientist, sees them as two groups ". . . comparable in intelligence, identical in race, not grossly different in social origin, earning about the same income, who almost ceased to communicate at all, who in intellectual, moral and psychological climate had little in common." He emphasizes that this split in the intellectual life of Western society also invades the practical life, and that, rather than producing the burst of creativity often resulting from such a clash of highly polarized cultures, it has spawned only a vacuum of misunderstanding and even disinterest.

Snow accuses nonscientific Western intellectuals of having ". . . never tried, wanted, or been able to understand the industrial revolution, much less accept it." I suspect that the defendants in this case might be sufficiently without concern for the implications of this accusation to give mute testimony to its validity. Such attitudes toward the industrial revolution and the scientific revolution which followed it in this country have even greater portent for all of us in the extent to which they affect the thinking of many of our top planners and diplomatic negotiators. Neither national nor international problems can be treated in the classical terms of men, money, and material, when the dominant force in the world today is so clearly that of science and technology.

If one agrees with the author that science ". . . is the material basis of our lives: or more exactly, the social plasma of which we are a part . . . ," and further agrees that most educated members of the nonscientific culture know nothing about it, we must face the question of why it is that we are not doing better at coping with the scientific revolution. Here Snow joins Hyman Rickover, James Conant, and a legion of others in placing the blame

on an educational system gone wrong. While not seeing anything like perfection in the Soviet educational system, he believes that they do have a deeper insight into the scientific revolution than we, and a correspondingly narrower gap between their "two cultures." His conclusion: ". . . we have to educate ourselves or watch a steep decline in our own lifetime. We can't do it—without breaking the existing pattern."

The last part of this Rede Lecture is concerned with what Snow sees as the main issue of the scientific revolution: the increasing difference in social and economic well-being between the people of the industrialized countries and those of the nonindustrialized countries, the disparity between the rich and the poor. Since this disparity is now quite obvious to the poor, and since the means for removing it, industrialization, is equally obvious to them, the disparity is unlikely to remain for long. Large inputs of capital and technical manpower will be required for the metamorphosis, and the author is convinced that we must find a means for supplying these inputs. If we don't the communist nations will, with implicit but nonetheless dire consequences for the West.

It can certainly be argued that Snow has greatly oversimplified his analysis of Western civilization in treating only two cultures—why not three or four or ten or a hundred? What is important, however, is that within any similar model which one might construct to his own taste, there will be found the same sort of cultural gap to which Snow refers. For example, it appears to me that a manifest cultural dichotomy, that between the culture of the profit-and-loss statement and the research culture, is of great importance to the readers of *Research Management.* What I have termed the P-and-L culture is built around the hard, dominating taskmaster embodied in the periodic profit-and-loss sheet, and it is often by necessity a culture of expediency and crisis. The research culture, in contrast, feeds on continuity and breadth of purpose. It is a tragedy all too common in modern American industry that these two groups, harnessed in tandem organizationally, often appear to be pulling the corporate wagon in opposite directions.

Just as with Snow's two cultures, we must recognize the dangerous gap between the P-and-L culture and the research culture, sometimes having so little in common both in motivation and language. Just as with Snow's two cultures, education is not the panacea here either, but it is something we can do and something which will, over the long run, be effective. The alternative is acceptance of the inability of American industry to adjust to the modern explosion of science and technology, and this, in turn, can only lead to slow attrition at best. Moreover, there are encouraging signs, and, here and there, one even finds a top management that looks on research as a working part of the corporate enterprise. From such a start can come the understanding

through which the gap may be bridged and the two cultures coupled in strength. Perhaps in this way, too, we may contribute in at least a small way to the solution of the problem posed by C. P. Snow.

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The Scientist in American Industry; Some Organizational Determinants in Manpower Utilization. Simon Marcson, 158 pp. Harper and Brothers, 49 E. 33rd St., New York 16, N. Y., 1960.

This book is described in its appendix in the following terms: "This study, however, emphatically is not represented as presenting a set of typical findings on industrial laboratories. It is a case history analysis of a laboratory, designed to accomplish a structural and functional analysis much as if it were a given single community." The appendix statement is much more correct than the title, and the study does communicate quite well the state of affairs between an industrial management and the scientists it employs. The laboratory studied is one of the larger operations of its kind, and there is reason to believe that the relations existing in it are also present at least to some degree in other research organizations which employ substantial numbers of scientists. There is also reason to believe that, since many of the relationships reported depend upon the scale of the research effort, other, different conditions prevail in the many small industrial units, which bear titles like "research department."

Marcson's main theme, which is clearly developed and supported by generous quotations from interviews, is that there is an important degree of misunderstanding and conflict between management and the scientists. This conflict arises because the scientist comes from a tradition in which he is privileged to choose his own topics of work. On the other hand, management operates in a tradition of passing on, amplifying, specifying, and executing decisions made by others.

Marcson also asserts that conflict is generated by different traditions as to who is expected to evaluate the worth of a man's performance. In the traditions of management, evaluation is done by hierarchical superiors; in the case of scientists, evaluation has come, traditionally, from peers and colleagues in science. Presumably these differences are based upon who has the requisite competence to make evaluations. One of the interesting aspects of this study is the way in which it reports the mutual concessions made by

both management and scientists to the values and traditions of the other. It seems that a *modus vivendi* has been achieved, but it also seems clear that both parties are not entirely pleased with it.

Marcson properly points out that the imposition of a management function on scientists is not confined to industry. That scientists have to live with management is increasingly becoming a fact of life in science generally. Management comes in with the establishment of research organizations at universities and generally as the scale and speed of research efforts increase.

Certainly Marcson's study is a valuable contribution to the understanding of organized science and presents a number of issues which scientists in organizations and the management of such organizations ought to be aware. It is worth reading by all those who are interested in science.

Marcson at various points suggests that the needs of the scientist for autonomy and for judgment only by colleagues arises out of the professional ethos which the scientist has acquired in the course of his training. The reviewer has a different point of view about professionalism in science, which leads to a different interpretation of the relationships Marcson reports. The reviewer believes that the essence of professionalism lies in the obligation of the professional to give his client what the client needs regardless of whether he wants it, and not whether or not the professional wants to perform the service. With this point of view, the striving of the scientist for autonomy in the choice of his service to his client independent of the client's need, is not a professional goal, however noble and valuable it may eventually be to humanity.

It is true that there is a considerable amount of independence in the professions of medicine, law, teaching, and the ministry, but it is an independence which is justified by the requirement that the professional be free to give his client the advice the client ought to have. In the case of science, there is not this justification for independence. Independence of the scientist comes out of the needs of the scientist. Independence is what makes the work of the scientist interesting to him. The independence of the scientist may or may not be good for the client, in this case the management which has a specific task in which it needs scientific assistance.

The striving of the scientists with management over independence, which Marcson reports, might be looked at as a fairly straightforward power struggle. In it, the scientists are attempting to gain the independence they desire, and management is attempting to keep them applied to the production of the products assumed to be required by the enterprise.

Each group brings to this conflict considerable resources; indeed, the resources of the scientists, in their strength, may be more than equal to management's than those of any other group with which management must deal

in the conduct of the enterprise. Because the scientists are in short supply, because they say they are ineffective unless certain conditions are met, and by their possibility of going back home to the university, they have been able to win substantial concessions from management, many of which are supposed to duplicate the advantages of the university.

The scientists' assertion that science is a profession may be looked at as one of their strategies in this power struggle. Since the recognized professions enjoy independence, the scientists assert that they too should be treated as professionals. Perhaps it is at least as appropriate to say that the conflict causes the "professionalism" of the scientists, as it is, to assert the reverse. The reviewer is not prepared to believe that a man with a new doctorate entering industry has absorbed any substantial professional ethos. It would be instructive to know whether or not the young scientist entering industry has any conception of himself as a professional. The belief that he is a professional may be a set of attitudes he acquires from his colleagues in the service of the corporation as he learns how to deal with management. The particular part of professionalism which is most useful to the scientists in their struggle is the pressure for judgment by professional colleagues. This is not necessarily a search for competent judgment but rather a strategy to gain autonomy. Judgment by colleagues in the professions is generally very lenient. Professionals are reluctant to make negative judgments of colleagues. Only when they tend to reduce the trust which the class of clients can have in the class of professionals, is there any negative evaluation of a colleague. Lawyers are disbarred, not for immorality per se, but when they tend to lessen confidence in lawyers generally.

While a sound case may be made that only a scientist can evaluate a scientist, they generally would not do this for management. If such a process of evaluation could be instituted, it would be a substantial victory for the scientists in meeting their need to pursue what interests them. The reviewer thinks it is a good thing that scientists do pursue what interests them. He believes that, in the long run, autonomy for them will generally benefit the corporation that permits it. However, he insists that such a course should be followed for its promise in mankind's game against nature, and not because the scientist says he is a professional—which, in essence, he may not be.

This sour view of professionalism has been presented as a reminder to the reviewer's colleagues in science and in industry that they should not look upon professionalism as a great dynamic force. They should rather try to understand what brings it about and what are the social justifications for the several characteristics of professional status.

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